Dr Bennett,
with best wishes
from the author.

REMARKS

ON

A CASE OF SUICIDE,

PUBLISHED BY

DR. P. D. HANDYSIDE,

IN NO. CXXXIV. OF THE EDINBURGH MEDICAL AND SURGICAL JOURNAL;

INTENDED TO SHOW THAT HE HAS ERRONEOUSLY ASCRIBED THE CAUSE OF DEATH TO

Air in the Organs of Circulation,

WITH SOME STRICTURES UPON THE THEORETICAL DOCTRINES ADVOCATED IN HIS MEMOIR.

(Read before the Royal Medical Society, 23d March, 1838.)

BY

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PREFACE.

The following remarks are printed almost *verbatim*, as they were read before the Royal Medical Society at its last meeting. I do not think that any of my objections were answered on that occasion; but as Dr. Handyside then stated his willingness to reply, in the event of my paper being published, and expressed a desire to have such an opportunity afforded, it is now submitted to the public. My sole object in bringing this communication before the Medical Society was the elucidation of truth, and the correction of what appeared to me to be errors of a dangerous nature; not merely as affecting the soundness of any physiological opinions, but also as connected, in an essential manner, with a point of much importance in medical jurisprudence.

I have first pointed out that there is no evidence that in this case the air found in the vessels was the cause of death, and have afterwards shown that, although we are not entitled to pronounce with absolute certainty upon this question, there is very great reason to believe that hemorrhage was the immediate occasion of the fatal result. The remarks which follow on the theoretical views of Dr. Handyside are merely intended to point out the untenable nature of his peculiar opinions, and not as a vindication of my own; for these I have already published in sufficient detail.

2, London Street,
March 26, 1838.

REMARKS, &c.

Dr. Handyside has, in the last number of the Edinburgh Medical and Surgical Journal, published an account of a case of suicide, in which he states that death was caused by the entrance of air into the vessels divided by the incisions which the unfortunate man inflicted upon his neck; and the same opinion is held by Mr. Watson, who mentions the case in his Treatise on Homicide. I was also under the impression that this was the cause of the fatal catastrophe, when my Thesis on the presence of Air in the Organs of Circulation was published, and in consequence, it was there briefly noticed.*

This, however, arose entirely from some essential errors in my information: and the account given by Mr. Watson, though said to have been furnished by Dr. Handyside, contains the very mistakes which misled me, and differs in the most material points from the narrative given in the Journal, a perusal of which convinced me, that there was not the slightest ground for attributing death to the air found in the blood-vessels.

Dr. Handyside informs us, that "on opening the pericardium, *the cavities of the heart were found collapsed*,† *and*

* Prize Thesis on Air in the Organs of Circulation, p. 35, Edin. 1837.

† A few lines farther on we are told that "the left cavities of the heart were found perfectly empty of blood, but *in a state of partial distension, like the other cavities of this organ from the presence of air*." How could the cavities of the heart be both "collapsed," and in "a state of partial distension?" In Mr. Watson's work it is stated that the heart and large vessels were "*moderately distended, but contained very little blood, and that in a frothy state*," p. 109.

nearly empty. The right auricle was perfectly empty of blood, with the exception of a minute coagulum at the foramen ovale. This cavity contained some atmospheric air." (p. 211.) The coronary, femoral, popliteal, and other arteries, as well as various veins in different parts of the body, when "placed under the surface of water, gave vent on being opened to several globules of air," (p. 212.) In the concluding remarks, when speaking of the medico-legal bearings of the case, it is stated that "*the heart was perfectly empty of blood, and contained air alone ;*" but there is no hint thrown out, as to the possibility of something else than the air found in the vessels having occasioned death; although it is stated in the next clause, that other cases of death from this cause "have generally presented in this organ commixed with the air some blood either in a fluid or else in a frothy state." (p. 220.)

Nysten, who has made a number of most careful experiments on this subject, observes, that when death is the speedy consequence of the entrance of air into the organs of circulation, *the right auricle is always found distended with frothy blood.* Amussat in a report upon the introduction of air into the veins, which he lately read to the Royal Academy of Mecine at Paris, mentions that he killed many animals by allowing air to enter the veins spontaneously ;—and in reference to the state of the heart, he speaks in the following very decided language—"*On trouve constamment les cavities droites de cœur distendues.*"*

There can be no doubt of the correctness of the statements of Nysten, Amussat, and many other experimenters, that the distension of the right side of the heart is a constant occurrence.

With regard to the frothy appearance presented by the blood, it may be well to remark, that if the right auricle be very rapidly distended to such an extent as instantaneously to arrest its contractions, the blood and air are found unmixed ;

* Archiv. Gen. Jan. 1838. p. 115.

but this is a result which can only be obtained when a wide tube is used, and great force employed in the introduction of the air. The fact, however, connected with the frothing of the blood, which it is of essential importance for us to bear in mind at present, is this:—*When an animal is speedily killed by the introduction of air, and bubbles of this fluid found in the vessels, the right side of the heart is always distended with* FROTHY *blood.* Of the correctness of this statement I am perfectly satisfied from the results of numerous experiments, and it is an opinion amply corroborated by those previously performed by Nysten, Magendie, and many others.

In some of the cases published as instances of death from air entering the veins, there is no mention made of the distension of the right side of the heart—nay, we are occasionally told that the heart was empty. But surely these cases cannot be brought forward in opposition to the unvarying results of numerous experiments performed in different parts of the world.* They must either have been erroneously included in this class of cases, or the accounts with which we are furnished, must be very inaccurate: or, possibly owing to the slovenly manner in which the examination was made, the air was allowed to escape from the heart. Velpeau has recently given an abstract of all the published cases of sudden death ascribed to the entrance of air into the veins, and many of these narratives he considers as furnishing no evidence of the real cause of death. Others, from his statements, appear to have been published on vague hearsay evidence, and are essentially fictitious. In the long and interesting debate which followed the reading of M. Amussat's Report, M. Gerdy stated his conviction that a good many instances of sudden death have been errone-

* M. Barthélemy has stated that in some instances in which the experiments were performed on horses, he did not find the heart unnaturally distended with blood; but as M. Barthélemy has not alluded to an obvious source of fallacy, viz. the division of the large vessels at the root of the neck in laying open the chest, I suspect it was not in these cases provided against. I have ascertained that when any of these vessels are divided, the distended heart becomes rapidly collapsed.

ously ascribed to the entrance of air. From the foregoing statements we cannot doubt that the narrator of the case of suicide at present under consideration has committed a similar error. The facts already adduced seem so complete and conclusive, that it is unnecessary to enter upon the discussion of some minor topics from which collateral evidence might be derived. It is maintained then, that the simple circumstance of *the heart being found collapsed, and destitute of frothy blood, of itself affords sufficient proof that Mr. Doherty's death has been erroneously attributed to the air found in the blood-vessels after death.*

It is no very unusual thing in post mortem examinations to find air in the heart and vessels. At the *sectio* of a fever patient who died sudddenly in convulsions in one of Dr. Alison's wards in the Royal Infirmary, examined thirty hours after death, and during the late intense frost, I observed air mixed up with blood in the heart. The inferior vena cava was enormously distended with fluid and frothy blood. Unfortunately from the relations of the deceased hurrying the dissection, there was not time to examine the other vessels. I have collected many analogous cases in the fourth chapter of my Thesis. In some of them in which the right side of the heart was distended with frothy blood, and the patients died suddenly, it was suggested that the air spontaneously generated, might be the cause of death.

In the case of Mr. Doherty, from the number of vessels divided, and the short period during which he lived after inflicting the wounds, a considerable quantity of air must have entered the organs of circulation after death—indeed after the arrival of Dr. Handyside,—for he stated in the Society, that when he came, the body was lying with the face on the floor, and the vessels so compressed by the adjoining parts as to prevent the entrance of air. Had it been stated that there was no air found in the vessels, the assertion would not have been credited by any physiologist, because, as is well known, Dr. Parry has proved, that the arteries, soon after death, in virtue

of a peculiar property with which they are endowed, contract, and after a time regain their former calibre. Now, it is obvious, that in this way a vacuum must be formed; and if there be an opening, the atmospherie air will, in accordance with a well known physieal law, immediately rush in, when this dilatation takes place. As there were many arteries divided in the case of Mr. Doherty, it is I think clear, that through these appertures a good deal of air must have gained admission after death.

There are various ways of accounting for the presence of the air found in the vessels of Mr. Doherty, and were not the subjeet somewhat irrelevant to the point at issue, it would be interesting to enter more into detail; but, since it matters not to the argument whether the air entered during life or after death, I refrain. I have injeeted a quantity of air into the veins of rabbits, without producing death; and then in the eourse of one or two hours killed the animals, (whieh had by that time quite recovered from the effeets of the experiment, and were eating or running about the room,) when I found a greater or less quantity of minute globules of air, in the vessels over the whole body. When the rabbits were not killed till after the lapse of some days, the air was found to have disappeared. It is maintained, then, that the mere presence of air in the vessels is no evidence of its having occasioned death. The *only* proof of this, is the distension of the right side of the heart with frothy blood.

There is no aecount given of the examination of the head, and Dr. Handyside stated in the Society, that none was made. This is very curious, beeause he grounds an hypothesis upon the state of the vessels of the brain, whieh nevertheless he was eontented to leave unexplored. This is mueh to be regretted, for the omission renders reasoning upon some points of the case, to a eertain extent, unsatisfaetory. Nevertheless, let us now endeavour to diseover what really was the cause of Mr. Doherty's death. From the small quantity of blood which, as appears from the narrative, was found in the body—from

the heart being empty of blood, and all the large vessels nearly so, and also from the paleness of all the organs,—we are naturally led to believe that the quantity of blood lost is far under-estimated when stated to have been not more than a pound and a half. It is worth noting that the blood was on a carpet, a circumstance apt to lead to an under-estimate, as a good deal must have been absorbed, and consequently not apparent to the eye. However, admitting that the hemorrhage was to no greater extent than this, it was, I think, in the circumstances of the case, quite sufficient to cause death.

In the first place, it is important to bear in mind, that the blood was rapidly poured out from a number of vessels, simultaneously, or almost simultaneously, divided. Of the rapidity of the effusion there can be no doubt, for Dr. Handyside tells us, that when he arrived ten minutes after the incisions had been made, the blood "lay partially coagulated upon three different parts of the floor of the apartment." Now, all writers on physiology state, that a small quantity of blood suddenly lost may induce fatal syncope; and every one in the habit of performing venesection must have been struck with the remarkable difference which exists between patients, in the amount of blood which they can lose before fainting takes place. When the body of the Princess Charlotte, who died of uterine hemorrhagy, was examined, *only twenty ounces of blood* were found in the uterus.

A woman, says M. Velpeau, was bled in the arm; "but eight ounces of blood had hardly flowed, when the patient uttered a plaintive cry and died! Nothing was found upon post mortem examination." What can we say of a case like this? "If death was not caused in this instance by the entrance of air into the veins," cries M. Maugeis, "what could have caused it?" Were I in this practitioner's place, my answer would be an easy one. I should content myself with saying, "*I know nothing about it.*"*

But there is a circumstance connected with the case of Mr.

* As translated in *Medical Gazette*, 17*th* March, 1838, p. 958.

Doherty which ought not to have been omitted in the memoir. It was known to the landlady, and many acquaintances of the deceased; and as the investigation was a judicial one, I should think also to Dr. Handyside. *Mr. Doherty had for some days been labouring under delirium tremens*, a disease in which, as is well known, the vital powers are easily depressed. Every one is acquainted with the very small quantity of blood which patients so affected are able to bear. When talking the other day of this subject to my friend Dr. Duncan, he mentioned a case quite in point, which came under his own observation. A man with delirium tremens was bled,—less than a pound of blood was taken, and he died in five minutes afterwards. When we consider, then, that Mr. Doherty was afflicted with this disease at the time he committed suicide, there is nothing improbable in the supposition, that a pound and a-half of blood suddenly poured out from a wound, dividing many large vessels in the neck, was the immediate cause of his death.

It still remains for us to notice that part of the memoir in which the general question, as to *how air when admitted into the veins causes death*, is discussed. "In tracing," says the author, "the *modus operandi* of the presence of air in the veins producing the foregoing train of symptoms and effects, although I may premise, that we know with certainty but little, yet we are justified in assenting to the well-founded general belief, strongly maintained by Nysten and Magendie, and supported by the late Sir John Leslie, that when the cavities of the heart become suddenly and violently distended with air, this organ cannot freely exercise its functions, but encounters much difficulty in contracting on such a powerful and elastic resistance. It cannot, however, be granted, that death is produced by the heart being prevented from propelling the blood which it contains into the lungs, as Nysten argues; for both the blood and the air, we have seen, are propelled, and in cases where, from the quantity of air that has

entered, a fatal result has ensued; not only has the heart been found empty, but the pulmonary vessels and the arteries of the system have at all parts contained air." Here we have an assertion; but as neither the opinions of authors, nor original experiments are cited in its defence, it must go for nothing; and there is not one experimenter whose name Dr. Handyside has mentioned that does not state an opinion directly the opposite; for, as has already been stated, they are all agreed in maintaining, that the heart is not only never found empty, but is always observed to be distended.

The next opinion canvassed, is one which we are informed is "*a common idea*," viz. "that the stoppage of the heart's action is occasioned by a loss of its contractile power from *overaction*;" and an experiment of my own is adduced to contravert this doctrine—a doctrine which most assuredly is not common; for though I have carefully examined the literature of the subject, I never discovered that such an opinion was held by any one; and judging from the identity of Dr. Handyside's references with those which I had previously given in my Thesis, his researches do not appear to have been more extended than my own, so that the refutation of this "*common idea*" is nothing more than a combat with a man of straw.

His own views now come to be developed; and a theory is propounded, which in some points certainly lays claim to novelty; but is upon the whole but a modification of that long ago adopted by Bichat. As the details are somewhat involved and complicated, to prevent any misconception, his own words are subjoined.

"It might," says Dr. Handyside, "probably be urged with greater propriety by those who seek to realize the old maxim, *cor primum vivens, ultimum moriens*, that the complete stoppage of the sanguiferous supply to the texture of the heart along the coronary arteries, is the real cause of death, seeing that Sir Astley Cooper's recent experiments go to prove, that the withdrawal even for a minute and a-half of the supply of

blood to the *mesocephàlon* and the spinal marrow, above the origin of the phrenic nerve, by compression of the vertebral and carotid arteries, is fatal. The notion must be allowed to be fair and reasonable, that the function of the heart may be similarly arrested by the withdrawal differently effected of the sanguiferous supply to its texture." How such a notion ean be eonsidered "fair and reasonable," is somewhat difficult to understand; for as every one knows the contraetions of the heart go on for sometime after the withdrawal of the sanguiferous supply from its texture—they go on for a period much longer than that which elapses between the introduetion of air into a vein, and the death of the animal experimented upon. But who is ignorant of the fact, that after the heart is eut out of an animal, and the coronary arteries consequently become *filled with air*, the muscular contractions of the organ are frequently seen to go on for a considerable space of time.

To proceed, however, with our quotation:—"Yet, being aware," says Dr. Handyside, "that this temporary failure of the circulation is not adequate to explain the circumstance of the rapidity of death being always in the exaet ratio of the suddenness and violence with whieh the heart is distended with air, (as the considerations already offered respecting the suspension of the heart's action may have served to show,) we are constrained to look more narrowly into the probable cause of death, and inquire, if, in such a case, the encephalon and spinal chord receive any supply of blood at all: and now finding that impossible, and next comparing the phenomena which supervened on the admission of air, with those symptoms and results whieh characterise several anormal states wherein a similar derivation of blood from the head occurs, we accordingly at once recognise the analogy, and may thence be led to adopt, as, at least, probable, such a view of the eause of death, as follows."

"The cessation of the heart's action must operate injuriously, first, on the maintenance of the function in those organs which require an uneeasing and uninterrnitted supply of blood.

As the encephalon receives about an eighth of the blood of the entire body, the prejudicial effects of a very slight want of balance in the proper amount of which, are so frequently presented to our view in the phenomena of syncope, and as the encephalon, and that part of the spinal cord, so essential to circulation and respiration, above the origin of the phrenic nerve, are supplied with blood almost exclusively, along the vertebral and carotid arteries; it is easy to understand how these parts of the nervous system are, by the distension of their vessels with an elastic fluid, wholly deprived of their supply of blood. And if the heart's action at the same time be not maintained sufficiently long, first, to circulate all the air it contains, and then to recommence the discharge of its appropriate fluid,—a result equivalent to the complete division by the knife, of that essential part of the nervous system must follow, —namely, the instantaneous cessation of all the automatic movements of the system."

"This view seems the more probable, from considering the interesting experiments already adverted to, and which I had recently the favour of witnessing Sir Astley Cooper perform. In all these experiments the manual compression for the period of a minute and a half of the vertebral and carotid arteries in rabbits, was followed by death, preceded always by violent convulsions, similar exactly to those which have been observed to occur on depriving the parts of the cerebro-spinal axis, on which these vessels ramify, of their supply of blood through means of the admission of air, either casually on the operating table, or intentionally in experiments on animals."* (Pp. 217, 218, 219.)

From what has now been quoted, it appears, that Dr. Handyside has entirely mistaken the nature and object of Sir Astley Cooper's experiments; for he merely states that "they go to prove that the withdrawal even for a minute and a half of the supply of blood to the mesocephalon and the spinal marrow.

* This is certainly reasoning in a circle.

above the origin of the phrenic nerve, by compression of the vertebral and carotid arteries, is fatal !" This required no proof. The object Sir Astley had in view, was to determine the order in which the vital functions were arrested, and it is from their elucidating this point that so much interest attaches to them. He found that *respiration was at once arrested, and that the animals died almost without a struggle*, and not, as Dr. Handyside represents the case, in "*violent convulsions.*"* Now when a large quantity of air is thrown into a vein, the phenomena observed are very different. Desperate attempts at respiration are immediately manifested, and death is very speedily ushered in by terrible convulsions.

Thus we perceive that there is no analogy between the effects produced by the introduction of air into a vein, and the withdrawal of the supply of blood from the mesocephalon and spinal marrow above the origin of the phrenic nerve :—and if no such analogy can be drawn, it necessarily follows that Dr. Handyside's hypothesis has no foundation to rest upon. Therefore, in preference to it, and every other doctrine on the subject, I must still maintain that which is held by Nysten, Magendie, &c. &c. and now, I believe by every one, who has, with sufficient care, experimentally investigated the subject, viz. that the inability of the pulmonic side of the heart to contract, in consequence of its distension with frothy blood, is the cause of death when this is the immediate result of the introduction of air into the veins.

* Guy's Hospital Reports, No. III., Sept. 1836.

INAUGURAL DISSERTATION ON THE PRESENCE OF AIR IN THE ORGANS OF CIRCULATION,

By JOHN ROSE CORMACK,

PRESIDENT OF THE ROYAL MEDICAL SOCIETY OF EDINBURGH, &c.

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